AGRICULTURE, RURAL DEVELOPMENT AND RELATED AGENCIES APPROPRIATIONS FOR 1985

HEARINGS

BEFORE A

SUBCOMMITTEE OF THE

COMMITTEE ON APPROPRIATIONS

HOUSE OF REPRESENTATIVES

NINETY-EIGHTH CONGRESS

SECOND SESSION

SUBCOMMITTEE ON AGRICULTURE, RURAL DEVELOPMENT AND RELATED AGENCIES

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PART 6

AGRICULTURAL PROGRAMS

Printed for the use of the Committee on Appropriations

U.S. GOVERNMENT PRINTING OFFICE

33-493 O WASHINGTON : 1984

SCREWWORM

1. Problem
Although the screwworm has been completely eradicated from the U.S. and throughout a substantial part of Mexico, the challenge to research and action agencies is to continue to prevent reinfestation of this devastating pest which has caused tremendous losses to the livestock industry in years past. Progress in Mexico during the past year has been so noteworthy that a barrier zone may be established at the Isthmus of Tehuantepec by December 1984. Central American and Caribbean countries have expressed strong interest in continuing eradication and establishing a barrier zone in Panama.

2. ARS Contributions to Current Technology
The screwworm program is perhaps the premier example of fundamental research by ARS scientists that led to new eradication concepts and, finally, the development and implementation of an operational program by APHIS. This program has been an unparalleled success. Screwworm eradication provides savings to the U.S. livestock industry in excess of $350 million per year. From the inception of the program, ARS has provided research support. Virtually all technology now used in the Mexico-U.S. screwworm eradication program was developed by ARS scientists in cooperation with APHIS program personnel. This technology includes:

-- Fundamental research on screwworm and biology and development of the sterile male insect eradication concept.

-- Methods for handling, mass rearing, and releasing sterile flies.

-- Genetic characteristics of screwworm populations of different geographical origin, revealing no evidence of reproductive isolation mechanisms, real or potential.

-- Screwworm adult suppression system to complement the sterile male technique of eradication -- especially effective in emergency actions to eliminate introduced foci of screwworms from previously eradicated areas.

-- Improved diet and new rearing techniques to meet ARS' responsibility for providing a screwworm strain for use in APHIS' high-priority screwworm production facility and strain development program.

3. Present ARS Research
ARS conducts fundamental and applied research on screwworms, devoting 10 scientist-years supported by $1.498 million annually at two locations: Tuxtla Gutierrez, Mexico, and Fargo, ND. A limited amount of supporting research is also conducted at Weslaco and College Station, TX, and Berkeley, CA. Major research thrusts are:

-- Strain development; enhancement of the screwworm adult suppression system and attractants; genetic studies of populations and cytogenetic investigations; heterosis for better release strains; field ecology; all-male strains for mass rearing; nutrition; population dynamics; and chemical control.

4. Recent ARS Advances

-- A new formulation, Swormlure IV, has been developed and is being used by APHIS in screwworm traps and in toxicant baits to replace Swormlure II, formerly used in the eradication program in Texas and Northern Mexico but ineffective in Southern Mexico.

-- A new strain of screwworms was developed for APHIS. It is far superior to the former strain and has played an important role in the tremendous progress of the Mexico eradication program. Program officials have elected to retain this strain in the mass-rearing facility rather than introduce a new strain.

ENTOMOLOGICAL SOCIETY OF WASHINGTON

C/O DEPARTMENT OF ENTOMOLOGY
Smithsonian Institution
Washington, D.C. 20560

INVOICE AND STATEMENT

Date 20 July 1984

To: Dr. O. H. Graham
PO Box 267
Weslaco, TX 78596

Our Invoice No. 84-181

Our Account No.

Your Order No.

Rec'd. Aug 1
Mel will prepare
AD-700 for
P.O. to be
issued in
Weslaco

For:

ITEMS	AMOUNT
MEMOIR MISCELLANEOUS PUBLICATION of the Entomological Society of Washington PROCEEDINGS XX	
Subscriptions: 19____, Vol.___,	
Publication Charges: 19 84, Vol. 86, Pages 714-719	
Author(s): R. J. Brenner	
Title: An invivo flourescent marker for spermatozoa of the screwworm . . .	
Page Charges: 6 pages at $ 40 per page. (immediate)	$240.00
Reprint Charges: 200 reprints at $ 78/49 per 100.	127.00
Corrections: 4 Corrections at $ 3 per line.	12.00
postage	1.71
TO INSURE PROPER CREDIT, PLEASE RETURN 1 COPY OF INVOICE WITH PAYMENT, AND SHOW INVOICE NUMBER ON CHECK. Total–	$380.71

Please remit by check, draft or money order drawn in favor of ENTOMOLOGICAL SOCIETY OF WASHINGTON, to

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Thomas J. Henry

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
AGRICULTURAL PRODUCTS QUALITY RESERACH
P. O. Box 267
Weslaco, TX 78596

July 20, 1984

SUBJECT: Fly Security, ARS Lab, Tuxtla Gutierrez, Chiapas, MX

TO: O. H. Graham, Research Leader
USDA, ARS
Apartado Postal No. 544
Tuxtla Gutierrez, Chiapas, Mexico

I have received your memo concerning fly security at the lab in Tuxtla and share your concerns. I feel that I can be of help to you and can work this in with a work trip concerning the Hercon SWASS-Swormlure and the navel cord attractants work.

However, I have several suggestions for consideration prior to this.

1) We need an approved security manual and an ARS chapter, if needed, prior to any significant changes by ARS. This does not mean that you should not use those security measures such as hot rooms, pass through boxes, changing clothes, showers, viable form records, window screens, and the like that are already available.

2) I would hold off on putting in the false ceiling in the old part of the security lab. Instead, you may consider at this time to paint the present ceiling white. This would allow you to visually inspect for cracks and loose flies in that area.

3) Again, my suggestion is to go ahead at this point in preparing the security lab in a similar manner as that used at Mission. A program of this type possibly would be acceptable to the Security Committee. Again, this is a recommendation, not binding, and not an attempt to speak for the Security Committee. I do believe that a considerable amount of progress can be made without the approved security manual.

I will be in Tuxtla during the week of July 30 - August 3, 1984 and will discuss this with you.

Harold E. Brown

HAROLD E. BROWN
Research Leader

cc:

N. Pineda
R. Reichard
N. Meyer
J. Novy
B. Liebe
R. Bram
C. Heald
R. Lowe
F. Smith
A. Pagasa Garza
S. Cajero Avelar
W. H. Sudlow
J. R. Johnston

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

12 July 1984

Donald O. McInnis
Research Geneticist
Tropical Fruit and Vegetable Research Lab
P. O. Box 2280
Honolulu, HI 96804

Dear Don,

I returned from Chetumal (Quintana Roo) from a two week survey yesterday to be confronted by the morass of correspondence between/among you, Hugh, U.T., and C. Schaeffer. I have declined to become involved in the debates for several reasons, mainly because the work here, both laboratory and field, has proven far more interesting, even exciting, than left-overs from the 1978-1982 epoch. My purpose in writing this is two-fold. First I would like to bring you up to date on some of our work here and in conjunction with Fargo and Weslaco. Second, I would like you to (please) consider or reconsider a joint letter with Leo, Jim Whitten, me and you (or some combination of us) in response to the American Naturalist note submitted by Richardson, etc. This note, as have its predecessors, has nothing of value to the program, no documental facts, and a great deal of (to me) absurd ideas.

We finally have good (and improving) facilities here. The "nite club" is clean and becoming secure. We also have secretaries, accountants, two DEC computors (Rainbow 100) and good equipment. David Taylor and I, along with a newly hired Mexican biologist, have been working with eye mutants. We now have three eye mutants characterized. Two are independently assorting, recessive, and autosomal. The third is epistatic with the first as silver-eye, and is only expressed when homozygous in a homozygous yellow eye. It may be in a third linkage group. I might add that we have maintained these lines in as many as 20 rearing groups for up to 20 generations (20 months) without a single contaminant. We use the same rearing methods used in Mission, Tuxtla and Fargo with technicians, maintaining the lines. Since these are recessives, contamination would be readily visible.

As you know, Jim Whitten's group in Fargo has devised a method and is currently completing work on descriptions of polytene chromosomes of screwworm. They have yet to find a single inversion, translocation, deletion, or anything else to indicate chromosomally based reproductive isolation among their numerous lines, including those which both Jim and I have found to produce conditionally sterile male hybrids.

My own crossing work, using lines collected by you in Michoacan-Colima, Peterson in Oaxaca, and Brenner and I in

Tapachula, showed a frequent appearance of male hybrid sterility when crossed to the same highly fertile line from Michoacan. My predictions that the program would be thwarted by this seemingly sympatric reproductive isolation fell on deaf ears. Rightfully, since by the time of completion of the third replicate, screwworms had been eradicated from the source regions for all of these lines. Taylor and I did use this evidence to argue against fly choice type strain development, since this sterility could remain largely hidden during strain construction. Except for age and size effects, we have found no assortative mating in cages. For developing the most recent strain, we instead used a method involving reciprocal forced crosses of all combinations of lines in four groups of four lines each. Then we allowed all hybrid lines to expand one generation, disgarding any female line producing sterile hybrids in any cross. We then pooled the hybrids in each of the four groups and again did forced crosses reciprocally. These groups were then expanded and pooled. This scheme had the advantage of discovering one "dysgenic" line which was omitted from the strain due to sterility and another line which was omited due to an eye mutant. These mutants frequently do not show up among the F-2's of newly colonized material, due to mortality, but a few tend to be found during the sexing procedures. When we compare fly choice lines with forced cross ones, both methods have produced excellent (A-82-flychoice; Sinaloa-forced) and mediocre or poor lines (Tex-Mex-flychoice, V-81-forced). Naturally we would prefer to know what makes a good line - especially whether there is any genetic variation in natural populations related to mating behavior and whether this may be used in strain construction. This is my highest priority now.

My major emphasis now is on cuticular hydrocarbon work. Harold Brown and I now have a complete GC system in place in Weslaco. We are looking at sex, diet, time in colony, age, mating status, and geographic source as well as possible heritable variation in cuticular hydrocarbons. I would like to eventually build isolines with various components absent and eventually use cuticular hydrocarbons as genetic markers and as applied methods to examine strain aging in the plant. In Chetumal, we (myself and two techs) collected and reared to pupae 100 lines from masses taken at 8 pens. We will probably build the next strain from this region. The region is east of nearly all of Guatamala, most of El Salvador and a bit of Honduras, so it represents a likely next target area.

In addition, Mackley is continuing the attractants work with slow release plastic formulations. Bill Rubink is working on life tables, parasites. and oviposition behavior, using natural populations. John Welsh (TAMU but working and living here) is looking at migration at higher elevations. Augustine is doing his protein and sugar preference work. David Taylor is also doing a great deal of electrophoresis work. In general, this laboratory is going at full capacity.

I guess the idea I'm trying to convey is that most of us here feel

that the U.T. hypothesis may have, 3-5 years ago, had some merit as a hypothesis. Your work cast considerable doubt as to its validity, and program success has completely discredited the arguments. We now have technologies including polytenes, sperm markers and probably cuticular hydrocarbons which, as we are applying them, are leading to sound, basic and applied, knowledge of the type necessary for further success. For this reason, the appearance of the American Naturalist article is not only silly, anachronistic, and immaterial to the program, but conveys the "ARS is charging into the tropics and inviting disaster" image. The contention that they are "currently completing an analysis of this data" from Arriaga (after three years!) has to be a joke. At any rate, would you consider communicating to Dr. Prout that ARS and the commission have:

1. Left this argument far behind both in program success and research progress.

2. Repeatedly shown predictions of the U.T. group to be incorrect.

3. Decided that three years (at approx. $50 million/yr, whether the program advances or not) is a bit long to delay a program to await analysis of a single field test.

You might also consider that screwworm research is not a "Christmas vacation" enterprize. People unwilling or unable to work for months at a time, full time, to learn Spanish (or other local languages), and to learn to collect, identify, rear and generally know their species are not very useful to the program.

I assume your family is doing well, also, when is your Drosophila (Hawaiian that is) research going to begin? I can guarantee you'll find cloud forest leaf breeding Drosophila research to be less stressful than "endangered Diptera" among the Calliphoridae or tephritids.

Sincerely,

Bob Mangan
Research Entomologist

cc. R. A. Bram
H. E. Brown
L. E. LaChance
D. Taylor
C. J. Whitten

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

July 27, 1984

SUBJECT: Screwworm Rearing Tests on the Production Floor with Gelling Agents

TO: Dr. Nazario Pineda Vargas
Dr. Robert E. Reichard

In this letter report I will summarize the current status of basic research and practical tests with gelling agents for screwworm larval rearing media. On July 24 Dr. R. L. Harris of the ARS Veterinary Toxicology and Entomology Research Laboratory in College Station, Texas arrived in Tuxtla and that afternoon he and I, in accordance with an agreement made with Dr. Luis Hernandez and Mr. Raul Marroquin that morning, went to the production floor and set up one vat each with two new gelling agents which had earlier shown considerable promise in small rearing trays in the ARS laboratory.

The tests were started by mixing 100 grams of Water Lock E1195-4.1 (Grain Processing Co., Muscatine, Iowa) with 10 liters of the standard production diet (Vat 1) or 100 grams of Water Lock A-400 (Vat 2), placing the resultant gel on half of a vat, and seeding each vat with one tray of 48-hour-old starting room larvae. New diet was added to each vat as follows:

July 24 - 9:30pm	10 liters	
25 - 10 am	10 "	
25 - 10 pm	20 "	a/
26 - 10 am	20 "	a/

At about noon on July 26 the test was abruptly terminated when the vats were emptied by production workers, presumably on orders from the Mexican Chief of Production. This made it impossible to obtain data on larval size and yield but Dr. Harris and I are confident that the larvae, which by that time were Group 6, were larger than larvae of the same age on acetate-hydroponic and, even more striking, no larvae were leaving the two gel diets at a time when significant numbers of larvae were prematurely leaving other vats in the same line.

It is most unfortunate that the yield data were lost as it appeared that larger numbers of larvae would be reared on the gel diet with a savings of half or more of the food product. Much more testing on the floor under actual production conditions will be needed but if a fully acceptable gel diet can be developed program rearing costs could be reduced by $2 to 5 million per year (50% of food product costs plus acetate cost plus labor savings plus engineering-maintenance savings

a/ - Concentration of gelling agent increased to 1.1%

(vacuum heads, expeller, and incinerator would be eliminated) minus the cost of gelling agent (estimated to cost about the same as acetate on an annual basis).

Here is a resume of what has been done on gel diets from the ARS viewpoint. For several years at joint meetings APHIS and Commission technical people have said that if the standard blood-egg-milk hydroponic diet could be placed on the larval rearing vats as a gel instead of pouring it on cotton linters or acetate, rearing would be facilitated and costs reduced. In 1982 I asked Dr. R. L. Harris, an internationally recognized expert on the laboratory colonization and mass rearing of Diptera, to help us develop a gel diet. His Laboratory Director, Dr. D. A. Witzel, very graciosly agreeded for him to work on this project at no cost to the screwworm program and my Area Director, Dr. J. R. Johnston has been fully supportive. Sine October 1982 Dr. Harris has made about 15 trips to Tuxtla, all paid for by ARS, and tested about 16 gelling agents and about 10 food products which might be substituted for blood, egg or milk. In addition he established a blow fly colony at College Station and used it to screen an even larger number of products. He has invested a great deal of time and effort in contacting industry and government experts in the field of polymerization and colloidal chemistry and obtaining information about the most recent advances in these fields.

In May 1984 Dr. Harris brought two new gelling agents to Tuxtla and tested them in the ARS lab on a small scale. The two Water Lock's had the advantage of easy mixing and improved physical stability and larval growth and yield were acceptable. The conditions under which he worked were far from optimum and we recognized the need for tests in full size vats held under standard rearing conditions. At that time he and I were both ready to discontinue testing because we felt that, in view of program success, it was unlikely that the Commission would be willing to replace an established rearing system with a new one.

In June however, Dr. H. C. Hofmann told both me and Dr. Harris that the need for a gel diet was more urgent than ever and that in his opinion the new gels should be tested in the plant, either on the production floor or in a Methods rearing area. On the basis of this request I arranged the purchase of larger quantities of the agents and urged Dr. Harris to interrupt his crowded summer schedule to come here in late July and do some quick preliminary tests with a small number of large vats to provide information that could be used to plan a larger scale test (20 to 100 vats) that would be sound and logical. Dr. Hofmann agreed with this approach and I confirmed our plans in a memorandum to him with copies to Dr. Reichard and Dr. Lowe (copy of memo dated 06-09-84 is enclosed).

When the gelling agents arrived in Tuxtla I began talking to Dr. J. D. Hoffman and Dr. Lowe about how these these tests could be run. During the week of July 16, in the absence of Dr. Lowe, I talked to Mr. Marroquin and among other things, offered to call Dr. Harris and cancel his travel if it would not be possible to do the tests the following week. Mr. Marroquin believed that the tests could be done, either in Methods or in Production. Dr. Lowe was of the same opinion on Monday may 23. On Tuesday May 24 we knew that the tests could not be done in Methods and Mr. Marroquin and Dr. Hernandez agreed that they could be done in Production if no more than 5 to 10 vats would be needed. It was a surprise therefore to receive Dr. Hernandez' Memo of July 25 (copy enclosed with copy of reply) after we had set up only 2 vats and a shock when the test literally "went down the drain" on the 26th.

Other than the cost of the travel and the loss of a week of Dr. Harris's time in the middle of a busy summer, no great harm has been done. The question is, where do we go from here? In my opinion the gel diet has too much potential to abandon it at this time. If you agree will you please instruct Commission officials in Tuxtla in writing as to what you would like them to do with the gel diet and the extent, if any, of ARS participation in future tests.

O H Graham

O. H. Graham
Location/Project Leader

Enclosures

cc: J. R. Johnston
R. A. Bram
R. L. Harris
H. E. Brown
C. J. Whitten
R. E. Lowe
H. C. Hofmann

ENCLOSURE 1

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

June 9, 1984

SUBJECT: Further Research on Gel Diets

TO: Dr. H. C. Hofmann

Dr. R. L. Harris and I are very pleased that you and other Commission officials continue to be interested in tests of gel diets as screwworm larval media. This interest is very timely as Dr. Harris tested two new gelling agents in May that seem to offer more promise than those he previously tested.

On the basis of your interest and the possibility that larger scale tests could be tested by Dr. Harris and other ARS people either inside Methods or on the production floor, we have ordered 20 kg. each of the following gelling agents from the Grain Processing Corp. of Iowa:

A - 400
E - 1195 - 4.1

While it will not be possible for Dr. Harris to return to Tuxtla to carry out these tests before September I will ask him if it might not be possible for him to make a quick trip in July to set up some small scale tests that would be preliminary to the more extensive tests suggested by you.

Hugh

O. H. GRAHAM
Research Leader

cc:
J. R. Johnston
R. A. Bram
R. L. Harris
H. E. Brown
R. E. Reichard
R. E. Lowe

COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO BARRENADOR
LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO
COMTA. 24803 KM. 2 CARRETERA A LA ANGOSTURA APDO. POSTAL 544
CHIAPA DE CORZO, CHIS.

Julio 25 de 1984
Ref: PM/0107/AC'

ASUNTO: Solicitud de información.

DR. O.H. GRAHAM
JEFE DE INVESTIGACION CIENTIFICA
A.R.S.
P R E S E N T E.

Por este conducto le solicito que en futuras ocasiones en las que desee utilizar instalaciones que pertenezcan al Departamento de Producción de esta Planta, tome en consideración que estamos regidos por un sistema bilateral, dentro del cual se debe notificar a ambas secciones. Por lo que le suplico que futuras ocasiones de manera oficial haga del conocimiento de esta Jefatura de Departamento las pruebas o los servicios que quiera efectuar, asegurándole que tendrá todo el respaldo y apoyo.

A t e n t a m e n t e

MVZ. LUIS HERNANDEZ GRUNENBERGER
JEFE DEL DEPTO. DE PRODUCCION
SECCION MEXICANA.

C.c.p. Sub'Directores de Producción.-Edificio.
C.c.p. Sr. Raúl Marroquín, Jefe del Depto. de Producción, Sec. Americana.
C.c.p. Técnicos Biólogos.-Planta.
C.c.p. Jefes de Turno.-Planta.
C.c.p. Archivo.
C.c.p. Consecutivo.

LHG'rms*

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab.
Apartado Postal No. 544
Tuxtla Gutierrez, Chiapas
Mexico.

26 de Julio, 1984.

DR. LUIS HERNANDEZ G.
JEFE DE PRODUCCION
SECCION MEXICANA
P R E S E N T E .

Estimado Doctor,

En contestación a su atenta carta PM/0107/AC de fecha 25 de Julio, opino que su inconformidad, muy claramente comunicada en su carta, resulta de un mal entendimiento o un cambio de opinión al último momento; un cambio no comunicado a nosotros.

En pláticas sostenidas con usted y su contraparte, el Sr. Raul Marroquin, llegamos a un acuerdo que nos permitan el uso de "5 ó 10 charolas". Le recuerdo que la única parte en la cual la Sección Mexicana, así como la Sección Americana, no estuvo de acuerdo fué cuando solicitamos 20 charolas. En esa plática quedamos en cambiar nuestra prueba para quedar entre las 10 que nos había permitido.

Al estudiar nuestras necesidades, decidimos llevar a cabo una "pre-prueba" con dos charolas. Informamos al Sr. Marroquin de nuestra decision de usar solamente dos charolas siendo esto menos que lo que usted ya había permitido.

Si posteriormente algo ocurrió para que usted cambiara de opinión, le hubieramos agradecido nos lo hubiera informado; no lo pensamos justo que después de darnos indicaciones de que se nos permitia la prueba, después de que llegó el Científico, y después de que empezó la prueba, comprometiendo nuestros recursos, que usted se moleste tanto y que de ordenes verbales (y sin informarnos) la eliminación de la prueba.

Sin más por el momento le aseguro mi estima personal y quedo de usted su atento y seguro servidor.

O H Graham

DR. O. H. GRAHAM
RESEARCH LEADER

cc.- Dr. Nazario Pineda
Dr. Robert E. Reichard
Dr. Ronald E. Lowe
Dr. Agustin Pagaza
Sr. Raul Marroquin

31 July 1984

Dr. Graham,

Here are two manuscripts that Chuck MacVean and I have coauthored. He and I have modified earlier versions, and we submit these to you for your comments and/or approval.

Concerning peer reviewers, Bill Rubink has consented to review the manuscript on the "Laboratory and Field Comparisons ..., and Bob Mangan has consented to review the ms on AO. I have no particular preferences for the second reviewers, although I thought about finding reviewers in Gainesville, if you do not have specific persons in mind.

If you have no changes to suggest before sending these for peer review, please give copies to Bill and Bob. I can send copies to other reviewers, or that can be done from here. If there are changes that must be made before review, send the copies to me; I'll revise them and return them to you for reevaluation prior to peer review.

Rick

R. J. Brenner
Research Entomologist

August 1, 1984

Dr. Nazario V. Pineda, Director
Dr. Robert E. Reichard Co-Director
Mexican-American Commission for the Eradication of the Screwworm
Leibnitz No. 20
APDO Postal M-2890
Mexico D.F. 06000
MEXICO

Dear Sirs:

In September 1982, Dr. Graham requested my assistance in developing a gelled media for rearing screwworm larvae. I understood that this research was needed and in fact requested by program officials.

It has taken over one and one-half years to develop a gelled media that may be satisfactory. During this time, I have made several trips to Tuxtla to conduct tests as well as conducting tests at College Station, TX. This research was done only by sacrificing my biological control research.

I have a hard time understanding why the tests being conducted in production were terminated. It consisted of only two vats and the larvae were equal to or better than the larvae on the adjoining vats.

If the program is still interested in the gelled media, I will return to Tuxtla to help set up a large scale test, however, your full cooperation is necessary as well as technical assistance. I worked until midnight on the last test only to have it discarded. I cannot justify this type of input in the future without your cooperation, support, and assistance.

Sincerely,

ROBERT L. HARRIS
Research Leader
Biological Control Research Unit

bcc:
R. A. Bram, NPS
O. H. Graham, RL

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

August 8, 1984

Mr. Raymond L. Everngam, Jr.
Managing Editor
Entomological Society of America
4603 Calvert Road
College Park, Maryland 20740

Dear Mr. Everngam:

To confirm our telephone conversation of earlier today I would like to request the return of Manuscript No. E 84095, "Effect of Native Fly Dispersal etc" by D. O. McInnis, J. W. Mackley and R. D. Peterson II. This manuscript was sent to Dr. Dorothy Feir by Dr. McInnis on July 13 without consulting his coauthors and contains several misstatements, misspelled words and other errors. I apologize for the obvious inconvenience to you and the Editors of Environmental Entomology; our poor coordination is the result of the distance between Dr. McInnis, his coauthors, and me.

Sincerely,

O H Graham

O. H. GRAHAM
Research Leader

cc:
J. W. Mackley
R. D. Peterson II
R. A. Bram
J. R. Johnston
H. E. Brown

U.S. DEPARTMENT OF AGRICULTURE **SPEED-MEMO**

PART NUMBER	DATE
3	08-08-84

TO Dave Hoffman
Methods Tuxtla

FROM Hugh Graham
ARS Tuxtla

SUBJECT Correspondence with Bill Schultz

MESSAGE

While filing some correspondence I found some information sent to me by Bill Schultz which may be of interest to you. A copy is enclosed.

SIGNATURE HG

REPLY

SIGNATURE

DATE

(DESTROY THIS PART 3 UPON RECEIPT OF REPLY)

FORM AD-311 (REV. 5-68)

☆ U. S. GOVERNMENT PRINTING OFFICE: 1976 — 202-409/657

U.S. DEPARTMENT OF AGRICULTURE **SPEED-MEMO**

TO Dave Hoffman → Methods Tuxtla	PART NUMBER 1
FROM Hugh Graham ARS Tuxtla	DATE 08-08-84
	SUBJECT Correspondence with Bill Schultz

MESSAGE *(WRITE CONCISE MESSAGE. SIGN AND FORWARD PARTS 1 AND 2 TO ADDRESSEE. RETAIN PART 3)*

While filing some correspondence I found some information sent to me by Bill Schultz which may be of interest to you. A copy is enclosed.

SIGNATURE HGG

REPLY *(USE THIS SPACE FOR REPLY. SIGN AND DATE, RETURN PART 2 TO SENDER. RETAIN PART 1)*

Hugh Appreciate the papers you sent. They present interesting food for thought. I have kept them for future ref.

Regards

SIGNATURE Dave H DATE Aug 13-84

FORM AD-311 (REV. 5-68)

U.S. DEPARTMENT OF AGRICULTURE **SPEED-MEMO**

FROM Dave Hoffman
Methods Tuxtla

TO Hugh Graham
ARS Tuxtla

PART NUMBER 2

DATE 08-08-84

SUBJECT Correspondence with Bill Schultz

MESSAGE

While filing some correspondence I found some information sent to me by Bill Schultz which may be of interest to you. A copy is enclosed.

SIGNATURE DHH

REPLY

Hugh Appreciate the papers you sent. They present interesting food for thought. I have kept them for future ref.

Regards

SIGNATURE Dave H

DATE Aug 13-84

FORM AD-311 (REV. 5-68)

08-07-84

MEMO TO ALL SCIENTISTS:

Harold Brown mentioned today that Bill Sudlow is processing a proposal to finance screwworm eradication on Jamaica with PL-480 funds.

OHB

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

August 8, 1984

SUBJECT: Manuscript, "Mass-tagging of Colonized Screwworms (Diptera: Calliphoridae) with a Fluorescent Marker of Spermatozoa"

TO: Dr. Robert L. Mangan
Research Entomologist
USDA-ARS Screwworm Research
Apartado Postal #544
Tuxtla Gutierrez, Chiapas

A copy of the subject manuscript is enclosed for your review. You are asked to recommend revisions and changes, based on your experience and knowledge, that will improve the technical content and clarity of the manuscript. Please sign and date the review form and return it along with your comments as we must have documentation of review by peers of the author(s).

If you find you cannot review the manuscript within three weeks, please return it immediately and indicate that it has not been reviewed.

We appreciate your efforts to improve our manuscripts by your constructive criticism. Thank you for your time.

O H Graham

O. H. GRAHAM
Research Leader

Enclosures

MANUSCRIPT PEER REVIEW

MANUSCRIPT TITLE

Mass-tagging of Colonized Screwworms (Diptera: Calliphoridae) with a Fluorescent Marker of Spermatozoa

AUTHOR(S)

Richard J. Brenner and Charles M. MacVean

REVIEWER'S NAME Dr. Robert L. Mangan	FOR PRESENTATION AT
TITLE Research Entomologist	PROPOSED PUBLISHER/JOURNAL
LOCATION Tuxtla Gutierrez, Chiapas, Mexico	J. Med. Entomol.
SIGNATURE / DATE 10 Aug 84	PUBLICATION RECOMMENDATION ☐ Acceptable as is ☐ Acceptable with revision ☒ Unacceptable

COMMENTS (Attach additional sheets as needed)
Authors must respond to specific comments on technical content and quality, by notation in the margin of this form or by attachment of a written response.

This note presents a progress report on the fluorescent sperm marker previously published. Emphasis is on feasibility of mass rearing screwworms by incorporating acridine orange into larval diet with the ultimate goal of having a sperm marker to compare strains. In comparison with the previous report, this paper is much narrower in scope both in what is new and how it is to be used. The proposed audience for this report would be extremely narrow. No field data at all have been collected showing acridine orange-marked flies are equally mobile and competitive with unmarked flies, thus managers of other control programs will have no assurance that "it works for screwworms". The last two paragraphs are crucial to the report; this work will have scarcely any value unless they are implemented. They are, however, directed only to perhaps a dozen APHIS personnel and half a dozen ARS folks, yet the other information has practically no value unless these recommendations are carried out.

I realize that both authors are trying to present results of experiments which I consider to be well done and with a possible great reward for the eradication program. Since both scientists have departed the Tuxtla program, someone else will have to complete this work. I recommend that this report be rewritten to give precise "cookbook style" methods for food preparation, rearing, and dissection along with an expanded field trial proposal. This type of report would then be suitable for an APHIS series report.

Coagulation Delaying Substance Helps Save Bat-Bitten Cattle

A substance that delays coagulation of the blood has been developed by the National Institute of Livestock Investigation of the Agricultural Secretariat and has already reduced cattle losses due to paralytic rabies carried by vampire bats by 70 percent, spokesmen said Tuesday.

Dr. Diodoro Batalla Campero, national coordinator for the institute, said there are 125 different species of bats but that only two of them feed on the blood of animals. The rest eat insects and fruits and are considered beneficial by fruit-growers because they help pollinate plants.

The two vampire species bite through the hide of cattle and feed on the blood. They return nightly to the same animals, reopen the wound, and feed again. The amount of blood loss is infinitesimal; the damage is done by the paralytic rabies virus carried by bats, which pass it on to cattle. The rabies virus can kill an animal within two weeks.

Loss of cattle in Mexico due to vampire bats was running at an average rate of 100,000 animals per year. Through the use of the new anti-coagulant, cattle deaths have been reduced by 70 to 75 percent.

Dr. Batalla Campero said that as use of the substance spreads across the nation, the loss of cattle will be reduced dramatically. He said the economic benefits to ranchers and the nation derived from saving some 70,000 animals per year will be substantial.

Batalla Campero said "Vampirinip" kills bats. He said an injection of the anti-coagulant into the bloodstream of an animal is passed to the bat when it feeds, killing it in a few hours by making the cattle's blood indigestible. He added that it is "remotely possible" that the two vampire species may be exterminated in Mexico in this way.

An alternative use is to apply the anti-coagulant as an ointment on cattle wounds. The bats reopen the wound and rub the ointment off on themselves while feeding.

Bats, said the expert, do not clean themselves but rather lick each other clean. Bats carrying the ointment containing "Vampirinip" thus pass it on to other bats.

Only blood-sucking bats are killed by the substance, as fruit- and insect-eaters do not come in contact with cattle.

Batalla Campero added that the institute has produced 300,000 doses of the anti-coagulant in its two forms. This will supply cattlemen in the states of Guerrero, Colima, Michoacan, Quintana Roo, Campeche, Chiapas, Veracruz and Mexico, where cattle losses due to vampire bats have been heavy.

The specialist said the success of the anti-coagulant in eliminating vampire bats has been proved and that export orders for the substance are being received from Central and South American nations, where cattlemen are facing the same problem with the bats.

Dr. Batalla Campero overlooked giving any credit to U. S. Fish and Wildlife Service who sent two scientists to work with INIP. These scientists devised the concept, and did all the basic and field work to prove that the concept was valid.

from Mexico City News
about 08-15-84

AGRICULTURE, RURAL DEVELOPMENT AND RELATED AGENCIES APPROPRIATIONS FOR 1985

HEARINGS

BEFORE A

SUBCOMMITTEE OF THE

COMMITTEE ON APPROPRIATIONS

HOUSE OF REPRESENTATIVES

NINETY-EIGHTH CONGRESS

SECOND SESSION

SUBCOMMITTEE ON AGRICULTURE, RURAL DEVELOPMENT AND RELATED AGENCIES

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ROBERT B. FOSTER and HENRY E. MOORE, *Staff Assistants*

PART 6

AGRICULTURAL PROGRAMS

Printed for the use of the Committee on Appropriations

U.S. GOVERNMENT PRINTING OFFICE

33-493 O WASHINGTON : 1984

SCREWWORM

1. Problem
Although the screwworm has been completely eradicated from the U.S. and throughout a substantial part of Mexico, the challenge to research and action agencies is to continue to prevent reinfestation of this devastating pest which has caused tremendous losses to the livestock industry in years past. Progress in Mexico during the past year has been so noteworthy that a barrier zone may be established at the Isthmus of Tehuantepec by December 1984. Central American and Caribbean countries have expressed strong interest in continuing eradication and establishing a barrier zone in Panama.

2. ARS Contributions to Current Technology
The screwworm program is perhaps the premier example of fundamental research by ARS scientists that led to new eradication concepts and, finally, the development and implementation of an operational program by APHIS. This program has been an unparalleled success. Screwworm eradication provides savings to the U.S. livestock industry in excess of $350 million per year. From the inception of the program, ARS has provided research support. Virtually all technology now used in the Mexico-U.S. screwworm eradication program was developed by ARS scientists in cooperation with APHIS program personnel. This technology includes:

-- Fundamental research on screwworm and biology and development of the sterile male insect eradication concept.

-- Methods for handling, mass rearing, and releasing sterile flies.

-- Genetic characteristics of screwworm populations of different geographical origin, revealing no evidence of reproductive isolation mechanisms, real or potential.

-- Screwworm adult suppression system to complement the sterile male technique of eradication -- especially effective in emergency actions to eliminate introduced foci of screwworms from previously eradicated areas.

-- Improved diet and new rearing techniques to meet ARS' responsibility for providing a screwworm strain for use in APHIS' high-priority screwworm production facility and strain development program.

3. Present ARS Research
ARS conducts fundamental and applied research on screwworms, devoting 10 scientist-years supported by $1.498 million annually at two locations: Tuxtla Gutierrez, Mexico, and Fargo, ND. A limited amount of supporting research is also conducted at Weslaco and College Station, TX, and Berkeley, CA. Major research thrusts are:

-- Strain development; enhancement of the screwworm adult suppression system and attractants; genetic studies of populations and cytogenetic investigations; heterosis for better release strains; field ecology; all-male strains for mass rearing; nutrition; population dynamics; and chemical control.

4. Recent ARS Advances

-- A new formulation, Swormlure IV, has been developed and is being used by APHIS in screwworm traps and in toxicant baits to replace Swormlure II, formerly used in the eradication program in Texas and Northern Mexico but ineffective in Southern Mexico.

-- A new strain of screwworms was developed for APHIS. It is far superior to the former strain and has played an important role in the tremendous progress of the Mexico eradication program. Program officials have elected to retain this strain in the mass-rearing facility rather than introduce a new strain.

Screwworm Research
Apdo. Postal #544
Tuxtla Gutierrez
Chiapas, MEXICO

14 August 1984

Dr. Donald McInnis
USDA-ARS Tropical Fruit and Vegetable Laboratory
P. O. Box 2280
Honolulu, HI 96804

Dear Don,

Glad to hear you and get your opinions about my opinions. First, to answer your questions: my understanding of the Fargo polytene work is that, at least as of June, they have found no variation, ie. no translocations, inversions, or lack of synapses in any of the material from Arriaga, Oaxaca, Michoacan-Colima, Villa Flores, or the Jamaica, 009, and Sinaloa lines. These include hybrids which I found to produce conditionally sterile males. As you know, there are a number of mechanisms to cause variations in both karyotype and banding patterns, including single mutations. Variants may be missed but, given the range of material sources, I am skeptical. Jim will be here in September to take Chetumal material back for further chromosome work.

My hybrid sterility work has results falling midway among your hypotheses. Hybrids were formed between a CIH34 line collected by you and four other of your lines (including two CIH lines). Seven lines from Oaxaca were also tested. In the first experiment, all females in backcross tests were checked for sperm. Less than 1% of females were scored as unmated. Jim Whitten got similar results so I decided not to dissect females in the second and third tests. Percent egg hatch and number of females ovipositing were the major factors causing male hybrids to be labelled "sterile". Since M. Crystal showed females mated with sperm-depleted (or semen-depleted) males to have reduced oviposition, the reluctance of isoline females to oviposit when mated to hybrid males is interpreted as a problem in male fertility. I should note that CIH34 was egged simultaneously and had no sterility problems. The results of the first test were as follows:

24 backcrosses were successful, producing adults
11 backcrosses did not produce pupae
11 backcrosses were discarded because of rearing problems
(Nine of the backcrosses discarded because of rearing problems involved hybrid males. I thought there might be a lab-caused problem in their failure.)

Of the 24 fertile crosses: 18 used hybrid females
6 used hybrid males

Of the 11 sterile crosses: 2 used hybrid females
9 used hybrid males

Pupal viability, development time, larval size, etc. were essentially the same for successful crosses when larval density was taken into account.

The second test suffered from a serious laboratory problem. A vat overheated and about one-half the hybrid lines were lost. This test had more total lines used. I would have discarded the data completely except that, for the 23 total surviving hybrid groups I had, 10 male hybrid and 10 female hybrid groups produced adult offspring and only two female and one male group were sterile. These hybrids were the same as those used in the first test. Again, there was no difference in larval size, development time, etc. The major result of experiment two was that these hybrids, which were stressed by a laboratory accident (heat shock as eggs and first instars), did not have reduced fertility.

The third test differed from the first two in that backcrosses were made to the non-CIH34 parent line. This was to check whether reduced fertility of the backcrosses was a CIH34 female problem. In this experiment the results were very similar to the first test:

30 backcrosses produced adults.
19 backcrosses failed to produce adults.

Of the 30 fertile crosses: 21 used female hybrids
9 used male hybrids

Of the 19 sterile crosses: 3 used female hybrids
16 used male hybrids

As to your questions about sterility - a sterile cross was one in which no viable adults were produced from 15-20 females. Since nearly all (>99%) of females checked, both ovipositing and nonovipositing, contained some sperm, at least some males of the 20 per cage were successful in copulation. Male survival in all cages was very high; generally by day 10 (egging) 3-5 females were dead, rarely were more than 2-3 males dead.

As you can see, this series of tests does not give the "all or none" type of data classical systematics requires. Even though sterility is mostly expressed in males (Haldane's Rule), the apparent strong environmental influence, ie. experiment two, is similar to Kidwell's recent hybrid dysgenesis work with Drosophila. In her work, however, sterility is strongest in female hybrids.

We are presently analyzing mating choice data. For these tests, males of the above strains are marked with acridine orange sperm marker, females are painted externally. A combination of two strains with and without markers are combined at five days of age with two reciprocally marked replicates. We have found two types of mating patterns about equally frequent. One is 100% mating by one strain of males in both replicates. The other is

random mating and with success rates of "dominant" strains being roughly equal between replicates. Right now I'm of the opinion that for these lines there is no prezygotic isolating mechanism but there are differences in mating vigor. We will repeat these tests with Chetumal material including some mate choice tests to some old lines (009, Sinaloa).

Concerning your nonagreement about methods of strain development. I was in error concerning the Tex-Mex strain. The strain I should have referred to was Mexico II, which according to DeVaney et al. (First Quarterly Report 1972, Kerrville, TX, p.18) was produced by mechanically mixing six strains from Vera Cruz, Oaxaca, and Guerrero. They clearly state that no crossmatings were done. In an appendix, which I believe you cite, by Whitten it is noted that there was considerable problem with egging this strain and, of course, the worst outbreaks recorded in the program occurred in 1972. The APHIS strain, also produced by mixing eggs, egged well but produced yellow-eye mutants "in substantial numbers". My major concern in citing these is that program folks here, especially some of the new production people, know only of V81 and A82 strains. I prefer to think that the excellent job these same people (especially Gersabeck, McVean and Marroquin) did in handling A82 when it was brought into production was the crucial factor.

My own opinion is that: 1) we have some evidence of hybrid dysgenesis among pure lines; 2) we have evidence that no mating preferences exist to avoid producing these hybrids in cage conditions; 3) eye mutant hybrids have been found in about 1% of isofemale lines and were very rare, or not observed, in the F2; 4) the program has shown that when 67-72 mg flies are used, eradication does not require much time or high densities of flies (ie. overcoming "mating barriers"). These reasons alone, I think, are sufficient to question the validity of fly-choice systems but we will perform a mate choice test using recently colonized material, hopefully with differing cuticular hydrocarbons. Another, I believe, factor which has been repeatedly discussed by ARS advisors (Beal and LaChance) is that a broadly based line should have desirable characteristics in terms of adaption to rearing conditions. Electrophoretic variation cited for Drosophila, mice, etc., does not hold up when the same systems are tested for screwworms. So, I question estimates of "coadaptation" in a species with few electrophoretic markers, no known chromosomal barriers to recombination, and no known behavioral barriers to protect coadapted complexes if they did exist. If strains show no mating preference in caged populations, differences in male aggressiveness would appear to reduce what variation there is in the population.

The suggestion you give for screening is sensible but remember the first generation in the laboratory expands the single egg mass lines, the second generation adults may be sexed and screened but since homozygotes may have developmental problems (orange-eye mutants were two days slower) screening will have to

continue into the third generation. Hybrids could be formed during the F3 and backcrossed in the F4. This would give candidates for the first fly-choice (F5) which will require at least three more generations to the F8 before the two generations to spread the lines (F10). Dave Taylor handed his VF84 strain over to Methods as F8 pupae after two generations of spreading. Therefore, there is a difference of two generations (= 1.5 months) in time and, as I argued above, there is no evidence that fly-choice, theoretically or empiracally, produces a better strain.

Finally, I will add a factor which I believe must be considered in comparing heterosis in Drosophila with that in screwworms. My own work with Drosophila brought into the laboratory compared with field populations showed that flies (from cactus and Hawaiian fruit) gained in size and correlated fecundity when reared on laboratory media. My experience with screwworms, growing larvae from recently (F3 and F4) colonized isofemale lines, indicates that the horsemeat diet reduces larval size by at least 30%. Horsemeat-reared flies weigh 60-70 mg, compared with sheep-wound-reared ones at 90-110 mg. I really doubt that what is coadapted in the wound is coadapted in meat. Dave Taylor has evidence supporting this. His VF84 first crossing group produced 71.8 mg (mean weight) larvae (SD = 5.5 mg). Thirty-nine crosses survived from 48 initial, with weight ranging from 60-80 mg. When these hybrids were selfed, mean size was 73.9 mg (SD = 6.4 mg) for the surviving 27 groups. In the next generation, mean larval size increased to 75.0 mg (SD = 5.0 mg). All these rearings were done with about 200 mg of eggs per vat. There is nothing I can see in these data to indicate that "coadapted complexes" are forming, then breaking-up.

Probably we can discuss this further in San Antonio. Dave Taylor will also be there. I have no intention of responding to the American Naturalist stuff. What you've done is probably sufficient. I will bug the people I know on the editorial board about the sinking quality of the journal. Things continue here. Brenner and Peterson have left so the majority of us are now the "new group". Unfortunately they tend to be as opinionated as some of the older scientists.

Sincerely,

Robert L. Mangan
Research Entomologist

cc. R. A. Bram
H. E. Brown
L. E. LaChance
D. Taylor
C. J. Whitten

United States Department of Agriculture
Agricultural Research Service
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August 2, 1984

Dr. Robert Mangan
Research Entomologist
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Mexico

Dear Bob:

Aloha! Thanks for your recent letter with all of the news from "down under." Sounds like you guys are keeping busy. Your letter touched upon a number of items which I would like to address, so I hope you'll bear with me. First, concerning a possible reply to Richardson's recent American Naturalist note (to be published ca. Jan. '85), I do not wish to waste my time further on any reply that I would author, for the following reasons. My response to Ellison and Richardson (Annals, in press) already addresses all of the points I would wish to cover; and, after some foot-dragging on the part of American Naturalist editors and/or Richardson, my request to have my Annals paper cited in Richardson's note will be honored. I feel this citation is both necessary and, hopefully, sufficient to give readers a balanced view. Further, in this series of letters back and forth to the American Naturalist, which would definitely top off your "morass of correspondence" (remember, I'm supposed to be working on tephritids!), Dr. Prout of the American Naturalist indicated that the Editorial Board would not look favorably on a rejoinder from me. In their view, the latter would not add anything new to the debate. As I mentioned earlier, I really am not interested in responding anyway, but their attitude seems a bit unfair (U.T. got a Science rejoinder to our USDA response in Science). Their attitude would probably apply to any response to Richardson submitted by any combination of USDA people. However, if you or someone else still would care to submit a note for publication and would like to have my name appear on it, then that's fine with me.

I've already sent Dr. Prout copies of the 2 Annals papers (U.T.'s and my response) for his edification along with a brief critique of their note, so I really feel that he has plenty of evidence and opinion to address the 3 points you hoped he would be apprised of. By the way, Leo has copies of all of the relevant correspondence on this for the screwworm record. I don't think you need to "sink deeper" into the details, but write me or Leo for copies of the correspondence if you want them. Anyway, please keep me posted on what you (and others) decide to do or

not do. As you might expect, I have grown weary of this matter, especially now that I'm supposed to be working on some other forms of "endangered Diptera." I, too, have work which is very exciting and interesting underway, both in the lab and in the field. As I mention in my Annals response, the time is long past for the U.T. guys and myself to turn the reins over to you and others who are actively involved in data collection and analysis (U.T.'s Belize samples notwithstanding!) Until now, U.T.'s persistence in continuing the debate has forced me to be a participant, but no more if I can help it.

Now, let me turn to address (in the sequential order of your letter) some of the items you mentioned concerning genetics research at Tuxtla. The isolation of 3 eye mutants could be valuable for linkage analysis, multiple mating tests, sperm displacement or competition studies, etc. Your observation about the lack of stock contamination in these mutant lines comes as no surprise to me (Jim Whitten and I countered U.T. repeatedly on that with chromosome and isozyme markers), but noting that for visible markers is icing on the cake.

Regarding the polytene research at Fargo, did you mean to say that no polytene chromosome variation at all has been found or just that no variation has been found to support a hypothesis of reproductive isolation (i.e., Fargo has found the expected Hardy-Weinberg numbers of heterozygotes in wild samples or lab mating tests). Frankly, I would be surprised if the former were true, especially for the sex chromosomes, unless variants are being missed for some reason. Chromosome variants I have described for mitotic tissue, especially those involving big differences in total length or arm ratio one might expect to be revealed in polytene preps, but I suppose that the different tissues and techniques involved could be extenuating.

The finding of sterile male hybrids is very interesting. I wish you had written me more about them in your letter. Perhaps you can later, after noting these comments. What is the percentage of total males which are sterile in the crosses that produce them? The potential mechanism(s) involved are related to these percentages, no? Also, of the sterile males, are they absolutely sterile and what is their relative viability? If the sterility is absolute (or nearly so) and the viability is good (esp. re mating) then possible program uses come to mind, especially if a sex sorting mechanism were developed. As for your prediction of program problems in Michoacan due to hybrid sterility, perhaps there was no resulting problem because there was no pre-zygotic (esp. mating) isolation between sterile and wild flies, but rather only post-zygotic isolation typified by hybrid sterility or breakdown. The latter would be academic if the sterile males mated and transferred competitive sperm, no?

Now, regarding a choice between "forced-cross" and "fly-choice" methods for developing strains. Forgive me if I appear below a bit defensive about the fly-choice method but I think you should expect that since I believe I coined the term and promulgated the idea within the USDA screwworm program. Let me say at the outset that I agree with you that both forced-cross and fly-choice methods could produce good or bad strains. Indeed, this could happen due to environmental factors (esp. rearing quality) alone, irrespective of any genetic consequences resulting from the method chosen to combine lines. With respect to your characterizations of past factory strains as good or bad quality lines, records I have which were compiled by Jim Whitten indicate that the Tex-Mex factory strain was put together by systematical (forced) cross-matings, not by a fly-choice method of simply combining pupae or adults (at the same stage of development) from 2 or more egg mass lines. Furthermore, I believe the Tex-Mex strain shouldn't be considered in a fair comparison of fly-choice or forced-cross methods, anyway, because it wasn't derived from a single, local population as were the other strains you mentioned (A-82, Sinaloa, and V-81). Of the 14 factory strains used since the start of the screwworm program (1951-53 in the Sannibel and Curacao experiments), 8 of these: MEX I, MEX II, Tex-Mex, FF8, 009, ARICRUZ, DE-9, and V-81 were definitely fully forced-crossed strains. The Sinaloa strain (one of your "excellent" strains) was actually a unique combination of 1/2 fly-choice and 1/2 forced-cross lines, each derived from the same 16 single egg mass lines. This occurred when pupal stock was supplied daily to APHIS from ARS. Ten days worth of each line were provided to APHIS to make the eventual factory strain. Of the remaining 5 factory strains, at least 2 [A-82 and APHIS (apparently)] were developed without forced-crossing (i.e., via some form of fly-choice method), while the remaining 3 strains (Original Texas, Florida, and Puerto Rico) seem to have been fly-choice type strains but my records are too sketchy to be certain. As for characterizing each strain as "good" or "bad", I believe such an exercise would be fraught with error due to the large number of variables involved (esp. rearing conditions of each strain, target native populations involved, and time in the factory).

What follows is a discussion of the rationale for using the fly-choice method as basically <u>insurance</u> against any degree of reproductive isolation involved in combining lines for a new strain. At this point in the eradication program, significant reproduction isolation in the field looks to be more and more just an academic concern since sterile flies have succeeded against several long-standing central populations. Yet, some form of assortative mating may occur in the field, in spite of laboratory evidence to the contrary. However, I do not wish to be a devil's advocate for it and hope that it never shows up for the sake of the program. The reason I encouraged the development of strains via a fly-choice method was to minimize the chance that a prospective factory strain would be developed which had dysgenetic properties resulting from hybridization and break-up

of possible co-adapted gene complexes. Now, as far as I know, no one knows if such gene complexes exist in screwworms and I certainly don't argue that they do, but they might. As a student of population genetics, you know that such gene complexes probably exist in many Drosophila species where they have been sought. I believe the program should be conservative in this regard as long as it is moving into new areas for sterile fly releases (e.g., Yucatan or Central America). Indeed, all other things equal or at least balanced, between forced-cross and fly-choice methods, certainly the amount of effort (esp. sexing) and total time involved in developing a strain is much less for the fly-choice method. In the interest of providing candidate strains quickly to APHIS for field testing and to free time for ARS scientists to do research projects, I think this factor would be a plus for the fly-choice method.

I agree that forced crosses would uncover hybrid sterility more easily than by merely mixing pupae in a fly-choice method, but such sterility as well as other undesirable traits (e.g. mutants) could be uncovered during several generations prior to mixing or crossing, during which time the various egg mass lines are screened. Previously (beginning at least as far back as DE-9) lines were screened for mutants, yield, larval size, fecundity, longevity, fertility, and genetic variation. Though forced crosses looking for hybrid sterility were not made then, they could be incorporated into the protocol for combining pupae in either a fly-choice or forced-cross design. Perhaps one or more "tester" strains (e.g. your Michoacan line) could be used to hybridize with candidate single egg mass lines which had survived the initial 1-2 generation culling period.

Though I don't happen to agree with all of the recommendations made by the Technical Panel on screwworm reproductive isolation (Kitzmiller, Markow and Wallace), I do support their view that the fly-choice method be used, as well as local source populations, for new strains. At least, I hope that future strains will be derived from local populations (as you appear to be doing with the Chetumal material) because the chance for hybrid disgenesis from incompatible genomes should be higher for allopatric populations than for a sympatric population, no?

Time and interest permitting, of course, perhaps you (or others) could continue and publish the work begun by Jim Whitten and myself (with the Sinaloa line) on rearing forced-cross strains and fly-choice strains starting with the same native egg mass lines. Rearing data was also gathered on a side-by-side comparison of the two methods for the Veracruz (V-81 and V-82) and Arriaga (A-81 and A-82) strains. To my knowledge, no big difference was detected in the laboratory for any of these 3 successive comparisons, but side-by-side field testing was never

done. Another suggestion (I realize it is easier said than done): How about experimentally testing the idea that factory strains derived from the F_1 progeny of a hybrid cross between two allopatric populations lines might be better than either line alone. Hybrid crosses which stop at the F_1 and avoid genetic recombination may yield progeny with significant F_1 hybrid vigor. The idea of having multiple factory strains for this purpose has been proposed before but the concept has never been adequately tested (including field assays) by ARS. Such research as proposed above might only provide mere "icing on the cake" type improvements in strain quality judging bythe evident success of current sterile release strains. However, as long as ARS has to develop strains for APHIS, it would seem wise to try experimental research in the process and help provide a sound basis for developing new strains. Along these lines, your ongoing highest priority research on possible natural variation in mating behavior and on cuticular hydrocarbon genetics sound interesting. I hope they prove fruitful for you as well.

As for my research, I am very busy investigating possible methods of genetically sexing medflies and studying the field behavior consequences and costs vs. benefits of implementing a males-only SIRM program. My other principal area involves strain improvement via eugenetic selection for factors such as fly size, starvation or dessication resistance, field survival tolerance, etc. Results have been encouraging so far. I assume you will be going to the San Antonio, E. S. A. meeting, too, so we will have time to chat there. As for Drosophila, at present, my efforts there are aimed only at destroying the pestiferous vinegar fly types which get into our tephritid diet!

Peggy is fine and working part-time as a nurse, while Brian has been a constant joy to us. Give my regards to Chris and all of your dogs. Come on over and see us sometime, O.K.? Aloha.

Sincerely,

Don

DONALD O. McINNIS
Research Geneticist

cc:
R. A. Bram, NPS, Beltsville, MD
H. E. Brown, ARS, Weslaco, TX
L. E. LaChance, ARS, Fargo, ND
D. Taylor, ARS, Tuxtla-Gutierrez, Mexico
C. J. Whitten, ARS, Fargo, ND

United States Department of Agriculture
Agricultural Research Service
Southern Region
Department of Statistics
Texas A&M University
College Station, TX 77843

August 17, 1984

RECEIVED AUG 2 3 1984

SUBJECT: Visit to Tuxtla

TO: Dr. O. H. Graham
Screwworm Research Laboratory
Apartado Postal 544
Tuxtla Gutierrez
Chiapas
Mexico

Hugh, Harold Brown talked to me after his last visit to Tuxtla. He indicated that some of the scientists were disappointed with the biometric help. I'm sorry about it. There is nothing intentional from this end. On the positive side, this feedback is telling me that the present emphasis of biometrics is not satisfactory, and that a greater or more effective program needs to be initiated.

As a start on the solution, let's begin with me visiting Tuxtla in the latter part of September. Perhaps around the 17th. I think we should plan on at least two visits per year.

The scientists at Tuxtla need the very best in biometric assistance and I can see where some deficiencies could have come in. It has been over a year since I have visited Tuxtla. There have been staff changes and I don't know the scientists and their programs as well as I should in order to give maximum assistance. Let's make the changes that are necessary so that the screwworm program can get the support that is needed.

Let me know about the timing for the proposed visit. Thanks for your help.

DelVar
H. DelVar Petersen

cc: H.E. Brown

08/24/84
I have invited DelVar to visit on date he suggests above — In the meantime, don't wait for him to do your routine statistical analyses —
OHG

RECEIVED SEP 1 9 1984

United States Department of Agriculture
Agricultural Research Service
Western Region
Western Regional Research Center
800 Buchanan Street
Berkeley, California
94710

29 August 1984

Dr. O. H. Graham
USDA - ARS Screwworm Research
Apartado Postal 544
Tuxtla Gutierrez, Chiapas
MEXICO

Dear Hugh:

This morning I telephoned your Tuxtla office, but I was told that you will be in Costa Rica until about the 10th of September. Therefore, I will mail this letter with the hope that you will be able to do a peer review of the enclosed Project Statement, INTERACTIONS OF INSECTS WITH MASS-CULTURE MICRO-ENVIRONMENTS.

This has been reviewed both within our laboratory and by Bob Jackson of the NPS at Beltsville. Now it is time for the "outside" peer reviews. Hopefully, there will be adequate time for both the mail and your personal effort so that this review can arrive back in California by September 30th. Even if that is not feasible, your review would be appreciated when available because I do value your breadth of experience and knowledge.

I also talked with Henri Charpentier about a review, and he is glad to do it from his "engineering" perspective. Although I consider the project oriented toward a metabolic process, in general, the past two years I have avoided the use of the word "engineering" because that term means so very many different things to the many different people associated with insect control, whether that association it be biological, chemical, genetic, SIT, or some academic discipline.

With best wishes, and

Sincerely yours,

Bill

W. G. Schultz

PRELIMINARY OBSERVATIONS ON SPIDERS IN THE SUMIDERO

Observations in the Parque Nacional del Sumidero during the collection period following the second ground release (6 July 1984) of sterile screwworms to study the dispersal of flies in the mountains indicated an increase in the number and diversity of spiders over the initial release (22 May 1984) and collection period. A decrease in the number of flies collected was observed during this second collection period. Increased vegetative cover, rainfall, predation, and decreased temperatures during this second period may have been factors in the reduction of the numbers of flies captured.

A study has been initiated to further examine screwworm predation by spiders in the vicinity of one of the release points. Following the third release (13 August 1984), several flies could be seen in each of the observable webs located within 50m of the release point and in spider webs in the vicinity of the individual traps. Spiders seen feeding on released flies were collected near release point number 5 during the first 3 days after the third liberation. Approximately 30 specimens representing possibly 10 species of spiders were collected after the individuals were observed trapping and killing screwworm flies in their webs. These specimens have been labeled and cataloged and will be mailed to taxonomists for identification. Flies are currently being released and individuals are being followed to ascertain their fate within the immediate area. In this study, a comparison is also being made between predation on flies supplied food before release and unfed flies. Upon completion of this test, an attempt will be made to collect all the spiders within the test area to to determine the species, number of species and relative abundance of species inhabiting the area. Notations will be made concerning observations of feeding on screwworms or the presence of screwworms in the webs.

John B. Welch

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Bill

W. G. Schultz

AGRICULTURAL RESEARCH

RESEARCH PROJECT PEER REVIEW

A– **Project Data and Peer Review Type** – To be completed by Research Leader prior to solicitation of peer review.

PROJECT DATA

PROJECT TITLE:	PROJECT NO.	DATE
Interactions of insects with mass-culture micro-environments	5325-20250-003	29 August 1984

REVIEW TYPE

AR or Non-AR Scientist ☐ AR Technical Advisor ☐ National Program Staff Coordinator ☐ Biometrician ☐

B– **Scientific Merit Review Criteria**

To be completed by all peer reviewers. Both qualitative ratings and narrative comments desired in spaces indicated; responses may be omitted on criteria peer reviewer does not feel qualified to address. Use additional sheets as necessary to provide responses.

1. Overall scientific value of proposed research project: Will the proposed project make a significant contribution to new knowledge, provide a better understanding of existing knowledge, develop appropriate new methodologies, or make valid contribution(s) to new technologies?

(Check one)				
Low	below	Average	above	High

NARRATIVE *(Please provide comments and explanation of rating)*:

2. Adequacy of the research proposal design: Is the proposal adequate and scientifically feasible with respect to the hypothesis, approach, and plan of work? Is the experimental design statistically sound?

(Check one)				
Low	below	Average	above	High

NARRATIVE *(Please provide explanation of rating and suggestions for improvements)*:

3. Adequacy of literature review and knowledge: Does the project statement provide an adequate review of the literature and demonstrate an appropriate awareness of the current state of the art?

(Check one)				
Low	below	Average	above	High

NARRATIVE *(Please provide comments and explanation of rating)*:

4. Adequacy of methods, equipment and personnel proposed: Are the proposed methodologies, equipment and personnel appropriate and sufficient to accomplish the objective?

(Check one)				
Low	below	Average	above	High

NARRATIVE *(Please provide comments and explanation of rating)*:

5. **Appropriateness of proposed timeframe:** Can the objective(s) be reached within the timeframe of the proposal and what is the probability of success? Can the objectives be restated so as to be achieved in the stated timeframe?

(Check one)				
Low	below	Average	above	High

NARRATIVE *(Please provide explanation of rating and suggestions for improvements)*:

6. **Scientific importance of the proposal and its relationship to ongoing research:** What is the degree of scientific relevance and urgency of the proposed research and will the results contribute materially to the success of other ongoing projects?

(Check one)				
Low	below	Average	above	High

NARRATIVE *(Please provide explanation of rating and suggestions for improvements)*:

7. **Extent of duplication of any (State, Federal or private) ongoing effort:** Does the project as proposed duplicate any (State, Federal or private) ongoing research or is it repetitive of previous research? If so, is this duplication/repetition desirable?

8. **Relative proportion of basic, applied and developmental research:** In the opinion of the peer reviewer, what percentage of the proposed research **is basic** in nature, what percentage **is applied**, in nature, and what percentage **is developmental** in nature?

C— **Program Merit Review Criteria**

To be completed by NPS reviewer and by other peer reviewers who feel qualified to address the criteria and questions.

1. Fit of proposed project to research priorities?

2. Need for research within context of National Research Programs, Technological Objectives, and USDA-ARS missions and goals?

3. Feasibility of completing the proposed research and achieving the objectives within the timeframe of the "need"?

4. Judgment of the cost/benefit of the proposed research? Adequacy of the funds and personnel requirements and facilities proposed?

5. Adequacy of the beneficiaries identified for the proposed project? *(Are the clientele and beneficiaries of this research properly identified? Are they appropriate to ARS missions and goals.*

D— Peer Reviewer Information (Will be provided to project leader unless specified to the contrary by peer reviewer.)

NAME	ADDRESS
TITLE	
SIGNATURE	
DATE	TELEPHONE NO.

PROJECT STATEMENT

A. TITLE: Interactions of insects with mass-culture micro-environments.

B. PROJECT IDENTIFICATION:

1. CWU: 5325-20250-003

2. PROGRAM PLAN CODE:

Objective	Approach	Approach Element	Weight
2	4	09.1	80%
3	5	03.1	20%

C. PROJECT LEADER: William G. Schultz
Engineering & Food Science Research Unit
USDA Western Regional Research Center
800 Buchanan Street
Albany, CA 94710
(FTS) 449-3380, (COM) 415-486-3380, (MESSAGES) 415-486-3701

D. OBJECTIVES: Develop fundamental knowledge on the effects of the physical, chemical, and microbiological micro-environments for the insect metabolic and physiological responses that occuring during the mass culturing. This new knowledge is to be applied to the development and testing of improved insect-culturing concepts.

E. DURATION: 5 years

F. SIGNATURES:

1. APPROVALS:

	Signature	Date
Research Leader:		
Laboratory Director:		
Area/Center Director:		
Regional Administrator:		

2. CONCURRANCE:

National Program Staff: _____ _____

PROJECT SYNOPSIS

A. TITLE: Interactions of insects with pest-resistant alfalfa-entry-cowpeas.

B. PROJECT IDENTIFICATION:

1. CRIS: 5325-30420-003

2. PROGRAM AREA CODE:

Objective	Approach	Approach Element	Mode
2	4	00.1	00
3	5	00.2	00

C. PROJECT LEADER: William S. Upton
Entomology & Plant Science Research Unit
Gulf Coast Research Center
P.O. Box 7
Ainsley, CA 92370
(213) 000-3000, (FTS) 615-456-2380, (WESTAMEX) 415-480-3700

D. OBJECTIVES: Develop fundamental knowledge on the effects of the physical, chemical and microbiological microenvironments on the host-insect relationship and physiological responses that occurred during the host selection. This new knowledge is to be applied to the development and testing of improved insect-resistant varieties.

E. DURATION: 5 years

F. SIGNATURES:

1. APPROVALS:

	Signature	Date
Research Leader:		
Laboratory Director:		
Area/Regional Director:		
National Administrator:		

2. CONCURRENCE:

National Program Staff:

G. NEED FOR RESEARCH

Insect mass culturing for sterile-male or biological-control applications is relatively in its infancy, though mass culture is an old art if one includes silkworm rearing. Although great strides have been made by the practictioners, there is still ample opportunity for further advancement to develop, improve, and rear mass multiples of individual insects which are cannibalistic, such as some moths, or mass groups which are compatible, such as flies.

As typified by current medfly and screwworm production, mass culturing accepts about a 50% larval mortality as an apparent result of the rearing-environment control. Overall, the present combined rearing efficiency and field efficacy from medfly egg through mass rearing to sterile-insect is likely 10-25%. For the larval-growth stage, the current state-of-the-art is based on the larvae seeking the best compromise available microenviroment within the diet. These diets are non-uniform chemically and physically, and they present a wide physico-chemical matrix of moisture, atmosphere, heat, temperature, nutrients, metabolites, pH, and microflora. The mobile adults and larvae under-utilize the rearing aggregate because they can seek a compromise environment. However,the immobile egg and pupa stages must accept the human-provided imperfect situation.

When larvae are reared in static, moist-solid diets, a thermodynamic relationship exists between the growth rates of insect and micro-organisms. For example, the range of microenvironments within a medfly larval-diet tray (5-kg) is normally outside the acceptable larval ideal; hence, the larvae migrate, overutilize some areas and under utilize others. Whereas, the microorganisms grow at an accelerated rate in the warmer locations of the diet which are not tolerated by the larvae. The management of this thermal-chemical microenvironment during egging, larval, and pupal stages controls in part the mass production efficiency, costs, insect quality, and ultimate field efficacy. Currently, this management is based largely on empirical relations between the room temperature and the average larval diet or pupal-media temperature. Room-temperature control is the primary independent parameter; the insect is the indirect dependent control. Future fundamental developments can change this to having the insect as the direct primary independent control. New concepts need to be developed for the mass rearing process to improve recovery and insect quality based on improved control of newly identified important micro -environmental parameters. The potential of insect specificity to habitat has scarcely been exploited in mass culturing, particularly the relation to artificial rearing.

Future advancement requires an expanded understanding of the fundamental relationship between insect metabolic and physiological responses to the mass-rearing conditions. Heat management is important because the resulting temperatures are a major rate and equilibrium force governing the bio-availabilities, metabolic reactions, and physiological results. Once the physico-biochemical micro-environmental and insect interactions are understood and can be controlled, the larval-pupal metabolism and physiology can be optimized for recovery efficiency, quality, cost, and field efficacy.

H. APPROACH AND RESEARCH PROCEDURE:

The purpose of this research is to develop new knowledge of the physico-chemical relationships between the insects and the artificial micro-environments during mass culturing. This research involves those insects mass cultured in artificial environments, both those currently reared and those which as yet can only be cultured in semi-artifical environments. New knowledge is to be applied to the development of improved rearing-process concepts for advancing insect quality and facilities design.

This research is a continuation of exploratory work in progress which started with a thermodynamic study using the housefly (Musca domestica) as a bio-model for Diptera. Subsequently, this was repeated in 1983 using the medfly (Ceratitis capitata) at the SARH-USDA mass-rearing factory in Metapa, Mexico. The results showed the convenience and validity of doing preliminary work on a model insect when based on metabolic similarities even though taxonomic details may differ. The original 1982 work concentrated on the thermodynamic relationship during the larvae stage and the control of that micro-environment. This project expands the previous physico-thermal work to interactions with biochemical and physiological aspects at the micro-environmental level.

Three general physico-biochemical areas at the micro-environmental level are to be examined for their effects and interactions with insect metabolism, adaptability, mortality, and quality parameters. These are: (1) the physical environment of temperature, heat generation, moisture, texture, homogeneity, atmosphere, and motion; (2) the chemical environment consisting of nutrients, bio-stimulants, insect and microbial metabolites, toxicants, and (3) the microfloral environment of contaminants and symbiotes. The previously developed dynamic incubator is expected to be a key experimental tool to provide controllable, uniform micro-climates for moist-solid diets.

Efforts will be concentrated on Diptera, Lepidoptera, and Coleoptera insects for which there are existing mass rearing programs in USDA. Primary insects are the medfly (Ceratitis capitata), screwworm (Cochliomyia hominivorax), codling moth (Cydia pomonella), pink bollworm (Pectinophora gossypiella), and boll weevil (Anthonomus grandis grandis). The initial focus will be a continuation of work in progress on the medfly and the screwworm.

Laboratory work will be conducted with the insects that are both indigenous and non-hazardous (eg. codling moth), or with innocuous insects which serve as useful bio-models for non-indigenous hazardous insects such as the black blowfly (Phormia regina) for the screwworm, and the housefly (Musca domestica) for the medfly. For the boll weevil, the alfalfa weevil (Hypera postica) appears to be a possible model. It is expected to proceed through F3 so as to take into consideration insect adaptability and physiological factors of size, reproduction, mobility, longevity, and similar field quality factors.

Based on the results of the insect-microenvironmental studies, the need is to identify key primary and secondary variables which can be useful in the control and management of production unit operations. Such variables differ with the insect. In the case of medfly larvae, a key principle is larval-diet heat management for temperature control which governs in part the metabolic directions and rates, and ultimately SIT efficacy. For screwworm larvae, the factor may be controlling metabolites in the diet. For the boll weevil, it may be those factors contributing to post field-release longevity.

This project requires a multi-disciplinary approach. To achieve this, internal cooperation will be between a chemist, engineer, microbiologist, and externally with entomologists. The emphasis will be on the insect-culturing "process" unit operations without emphasis on nuts-and-bolts of equipment or materials-handling. The previous work with interactions between insect and the micro-environment was done by the project leader, an engineer with a background in biochemistry and microbiology. The entomology specialty will vary with the insect and the specific capability needed for measuring the insect responses. In particular, the direct input of insect physiologists are very important part in judging the responses of insects in both lab and field. Chemical cooperation with the Biocommunication Chemistry Group at WRRC is a continuation from prior projects; this group is active in chemical volatiles associated with the medfly, screwworm, and other insects. The internal input of microbiology is also a continuation from prior projects; it is essential because the early thermodynamic work showed the basic relation to microfloral activity. Prior publications have shown the critical nature of the microflora control and their beneficial contributions to insects as symbiotes.

These disciplinary capabilities need to be simultaneously at the same site to work on the identical insect. Rate samples are transitory for volatile vapors, microorganisms, insect physiology, and culture conditions. The results of these samples and parameters need to be summed as a mass-energy balance to place the results in collective perspective. An example is the microorganism background differential between Mexico and Albany which illustrates the need to use samples from one location. The required research staff is at WRRC. In addition, the Center has a professional staff of diverse backgrounds which can be readily consulted for additional support to this project.

I. LITERATURE REVIEW

The literature shows mass-culturing technology progressing from a laboratory to a factory basis where the yield is expressed in tons of insects per week. Insect culturing is often divided into agents (predators, parasites, microorganisms) for biological control and insects for the sterile-male technique.

Current SIT factories are typified by the joint SARH-USDA medfly and screwworm facilities in Mexico producing pupae at tons per week, requiring hundreds of employees, and multi-million dollar yearly budgets (1,2,3,4). Other typical commercial and governmental mass rearings are for housefly, codling moth, pink bollworm, boll weevil, gypsy moth, corn earworm, melon fly, and many others (5, 6,7,8,9,10,11,12). Many of these rearings are for the sterile-male technique while others are for insect intermediates in rearing parasites or predators for the biological control of insects and weeds (13,53,54). This review is primarily directed to mass rearing for the sterile-male technique, but many rearing fundamentals apply to culturing for both SIT and biological controls (13,33). Beyond the silkworm, mass reared insects have been suggested for a variety of uses, from fuels to foods and feeds (14).

Rearing conditions and field efficacy interrelations were acknowledged early and remains an area to continue exploring and improving. This insect specificity to environment is still a major limiting obstacle in many cases, such as for many weed consuming insects. Inadvertent selection, adaptation, or mis-handling of the insect to artificial rearing that is not appropiate for field efficacy is a reality (47,49). Insect quality has been assessed in a variety of ways, and this continues to be an expanding field (15,16,17,18,19,20,21,22,23,25).

Nutritional requirements are an expanding area, both when viewed in vivo and in vitro as the environment modifies the bio-availability of the nutrients (17, 24,25,26,27,28,29,55). In conjunction with the nutrients are the feeding concentration, stimulants, and factors affecting the diet consumption (30,31,32).

Larval diets have been considered mainly on the average temperature until 1982, when larval heat and heat rate was first measured (34). Prior to this, there were mini-scale lab experiments on respiration and metabolic heats, but not as as function of insect progressive development and associated microflora (39). These temperatures associated with mass rearing were essential to the general establishment of factory-scale mass rearings through the adult, larva, and pupa stages (10,35,36,37,38,60).

The potential of integrating the various fundamentals of the physico-chemical micro-environment into the rearing system is still be exploited. Relating chemical volatiles associated with insect sexual and non-sexual stimuli with the microflora, plant, and other sources will further mass rearing (40,41,42,43, 44,45,46).

Cost of control programs are governed in part by rearing expense and the field efficacy. While the field damage is assignable, program costs continue to be a major factor when considering the options of biological controls, sterile-male techniques, chemicals, other combinations resulting in integrated pest management for such diverse problems as snails, water hyacinth, citrus, and cotton (50,51, 52). Longterm production of the boll weevil, medfly, and screwworm may make the reduced cost of fully artificial rearing attractive (1,4,9). For short-term production to permanently establish new field parasites and predators, a semi-artificial culturing may be more than adequate as for the Comstock mealybug (58).

J. COOPERATION

Cooperation will be continued, both external and internal to WRRC, with other scientists who can provide their additional multi-disciplinary interests in both the sterile-insect techniques and biological controls. Basic facts and concepts developed by us with amenable model insects can be subsequently correlated and applied to the production of harmful insects. Once done with several typical insects, the pattern can have a more universal application.

To date active cooperation has primarily been on medfly thermodynamics. Research thermal values by the project leader have been applied to medfly mass-rearing facilities design by APHIS for the Hawaii medfly standby-production facility, and by the U.N. International Atomic Energy Agency for the MISR-MED facility for Egypt. These are costly facilities for producing up to 10^9 insects/week (8-tons).

This cooperation followed the pattern of initial laboratory research on using the housefly as a medfly model. This work established the basic Diptera thermal-rate progression from adult, though larval growth, and through pupation, maturation, and emergence. Those thermal fundamentals were confirmed on medfly in four-way cooperation between the ARS, APHIS, SARH (Mexico), and UN-IAEA at the APHIS-SARH sterile-medfly production facility at Metapa (Chiapas), Mexico. This values have been applied to the air-conditioning designs mentioned above; for the MISR-MED facility, the value for the air-condtioning alone is about US$2,700,000.

Continuence and expansion of such cooperation is of mutual benefit to all four parties for design data, production revisions, cost evaluation, and guidence to relevent research for changing needs. The SIT and biological controls can be viewed as now in relative infancy to their future potential. Expansion of cooperation on other insects is underway.

Cooperation was discussed with Dr. Owen H. Graham, ARS Screwworm Research Leader at the Tuxtula Gutierrez facility. Some intial work has resulted at WRRC for the purpose of using an innocuous blowfly (Phormia regina or equivalent) as a model for the screwworm (Cochliomyia hominivorax). This model would be used to study the larval and pupal responses to rearing fundamentals without duplicating the work of either Graham's group or the APHIS Methods-Development personnel.

Cooperation has also been discussed with Dr. John P. Reinecke of the ARS Boll Weevil Laboratory of Starkville, Mississippi. Although the boll weevil has very different characterics than the Diptera, there are many similar metabolic characteristics. These metabolic similarities and dis-similarities provide the necessary contrasts to improve mass rearing of insects cultured under artificial environments and for their ultimate efficacy when released under natural field condtions.

K. RESOURCES

Fund Source:

Personnel Required:

1. Scientists

W. G. Schultz Process Engineer, GS-14	1.0 SY

2. Support

J. Turnbaugh Food Technologist, GS-9	1.0 PY

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Dr Graham

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
AGRICULTURAL PRODUCTS QUALITY RESEARCH
P. O. Box 267
Weslaco, Texas 78596

September 12, 1984

Dr. Samuel Rawlins
Department of Microbiology
University of the West Indies
Mona, Kingston 7,
Jamaica, W.I.

Dear Sam,

I have not forgotten your request for Swormlure-4 but have had some problems getting my contacts lined up. I have been unable to contact the principal individual.

I have talked to the air freight people in Miami (Jamaica Air Freighters) and they said, with the proper permits, they can ship this to Jamaica if I can get it to them in Miami. However, I am not having much success shipping the material to Miami. I will continue these efforts, in the meantime, please contact the Jamaica Customs and the Ministry of Agriculture concerning proper permits required for entry of this material to Jamaica. I will proceed as soon as I receive this info and/or permits. I propose to send 2 boxes containing a total of 6 liters/box or 12-500 ml bottles of Swormlure-4. The other alternative to this is to acquire the chemicals and prepare Swormlure in Jamaica.

I trust that things have gone well with you. Hugh Graham is planning on returning in December 1984. They have not picked his successor. We have been busy putting together our thoughts on some Central America research and probably will establish a worksite in San Jose, Costa Rica. I've not heard much from the Jamaican Proposal. I've talked to the APHIS people and they seem to think there is a good possibility of getting some PL-480 funding, however, I don't know.

I'm looking forward to hearing from you before long. Pass on my regards to Dr. Hilton, Vince, and others.

Sincerely,

Harold

HAROLD E. BROWN
Research Leader

Hugh Graham

United States Department of Agriculture
Agricultural Research Service
National Program Staff
Beltsville, Maryland 20705

September 12, 1984

Dr. Alberto B. Broce
Department of Entomology
Kansas State University
Manhattan, Kansas 66506

Dear Alberto:

I was, indeed, pleased to learn that there is a possibility of your taking a sabbatical leave in Panama during 1986 and, while there, resuming research on screwworms. As you may know, there is escalating interest among countries of Central America, as well as within our own livestock industry, to proceed with screwworm eradication to Panama and to establish a permanent barrier in that country. Since ARS is responsible for providing the research necessary for successful eradication, we are now initiating new efforts to conduct screwworm field investigations in Central America with particular emphasis on ecology and population genetics.

Should your sabbatical materialize, I would hope that arrangements can be made for cooperative research activities in Panama leading specifically to improved knowledge of the ecology and population dynamics of screwworms in the area of Panama adjacent to Colombia. Please keep us advised of your progress in establishing a sabbatical and of your research interests while in Panama. Best regards.

Sincerely,

Ralph A. Bram

RALPH A. BRAM
National Program Leader
Insects Affecting Man & Animals
and Parasitology

Ralph A. Bram

United States Department of Agriculture
Agricultural Research Service
Beltsville, Maryland 20705

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Screwworm Research Project
Apartado 544
Tuxtla Gutierrez
Chiapas, Mexico

AIR MAIL

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AIR MAIL

United States Department of Agriculture
Agricultural Research Service
National Program Staff
Beltsville, Maryland 20705

September 13, 1984

SUBJECT: Planning for Future Screwworm Research

TO: J. R. Johnston
O. H. Graham
H. E. Brown
C. J. Whitten

In preparation for my forthcoming trip to Tuxtla Gutierrez in November, enclosed is a flexible, tentative agenda of visits and planning meeting discussion topics. I would be grateful for your comments, additions, or modifications to this agenda.

Ralph

RALPH A. BRAM
National Program Leader
Insects Affecting Man & Animals
and Parasitology

Enclosure

TENTATIVE AGENDA FOR TUXTLA GUTIERREZ VISIT

Thursday, November 8

A. Individual visits with Screwworm Project Leader and Research Scientists.

B. Visit screwworm plant, including methods developments sites as time permits.

Friday, November 9

PLANNING FOR FUTURE SCREWWORM RESEARCH
(Graham, Brown, Whitten, Johnston/Murrman, Bram)

A. Research location priorities/initiatives in 1985 and beyond.

1. Tuxtla Gutierrez, Mexico.
2. Weslaco, Texas.
3. Fargo, North Dakota.
4. Costa Rica (define role of satellite laboratory).
5. Jamaica.

B. Special screwworm research topics.

1. Nutrition, diet, new media formulations.
2. Basic and population genetics.
3. Strain development.
4. Factory adaptation.
5. Ecological investigations.
6. Modeling and simulation.
7. Attraction.
8. Swormlure (Hercon contract).
9. Chemical control (regulatory treatments).
10. New ideas/approaches.

C. General topics.

1. ESA screwworm symposium
2. APHIS research priorities FY 87.
3. Screwworm Commission relationships, particularly methods development.

Saturday, November 10

A. Continue planning as necessary.

5 November 1984

JOURNAL OF MEDICAL ENTOMOLOGY
Department of Entomology, Bishop Museum
P.O. Box 19000-A
Honolulu, Hawaii 96817

Re: The ms. "Biology of a south Texas population of the blow fly Chrysomya rufifacies (Diptera: Calliphoridae), a recent immigrant into Texas" by Ring & McInnis

Dear Dr. Graham:

Receipt of this ms. is acknowledged with thanks. Please note that the reviews by Drs. Gagne, Richard, and Teel referred to in your accompanying letter were not in the package; please send them if you would like them included in the file.

Articles are subject to a charge of $65 per printed page. You will be notified as soon as the review process is complete.

Sincerely,

Jane Taylor
Editorial Assistant

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

Septiembre 14, 1984

ASUNTO: Ensayo del Uso de la Dieta de Gel en la Sección de Producción

PARA: Dr. Agustin Pagaza

Esta semana recibimos un envio de 60 kg. de Water Lock A-400, el agente gelatinoso para el medio de larvas del gusano barrenador (40 kg. de Lot 38170 y 20 kg. de Lot 38202). Este material esta a su disposición si desea usarlo todo o una parte, para ensayos en la planta. También, nosotros en ARS estamos dispuestos a ayudarle en estos ensayos si lo desean. Estoy seguro que podríamos arreglar que el Dr. R. L. Harris, nuestro cientifico mejor informado, esté aquí para observar y aconsejar si es necesario. Si usted decide preparar un protocolo podemos colaborar en su preparación aunque quedo dudoso de su valor (porque si nosotros supieramos exactamente como usar el material con óptimos resultados en la sección de crecimiento, el ensayo no seria necesario).

La técnica promete ser útil aquí en Tuxtla y en cualquier planta nueva de crecimiento que pueda ser construida en el futuro pero nunca sabremos con seguridad hasta que sea ensayado en Producción. Los ensayos podrán ser conducidos solamente en la Sección de Producción por el personal de Producción, pero nosotros en ARS estamos listos para ayudar en mucho o poco, como ustedes gusten.

September 14, 1984

SUBJECT: Trial Use of Gel Diet on the Production Floor

TO: Dr. Ronald E. Lowe

This week we received an additional shipment of 60 kg. of Water Lock A-400, the gelling agent for screwworm larval medium (40 kg. of Lot 38170 and 20 kg. of Lot 38202). This material is available to you if you wish to use all or any part of it in trials in the plant. Also, we in ARS will be very happy to help you with these trials if you would like to have our help. I am confident that we could arrange for Dr. R. L. Harris, our best informed scientist, to be here to observe and advise if this would be helpful. If you decide to prepare a protocol we would be glad to assist with its preparation even though I remain dubious of its value (If we knew exactly how to use the material for optimum results on the rearing floor we wouldn't need a trial).

The technique offers promise of being useful both here at Tuxtla and in any new rearing plants that might be built in the future but we will never know for sure until it is tried in production. Trials can be conducted only on the production floor by Production personnel but we in ARS are ready to help as much or as little as you would like.

O. H. Graham

O. H. GRAHAM
Location/Project Leader

cc:
R. L. Harris
H. E. Brown
H. C. Hofmann
R. E. Reichard
R. A. Bram

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
Screwworm Research Lab
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

September 21, 1984

William G. Schultz
Engineering & Food Science Research Unit
USDA Western Regional Research Center
800 Buchanan Street
Albany, CA. 94710

Dear Bill,

I regret that your letter of August 29 with a project statement enclosed did not arrive here until yesterday. In the hope that you may receive it before the 30th I have responded promptly and will mail copies by two routes. I assume that you do not need the project statement itself and am keeping it for our file.

As you can see I have reviewed the statement strictly from the viewpoint of screwworm rearing and have not commented on other species such as the boll weevil. In my opinion everything you learn from tests with Phormia will have to be proven in tests with screwworms but this does not detract from the justification for your project. At your location it will be feasible to study the basics of blow fly growth and nutrition with a degree of sophistication that would be impossible, or at least less feasible, at any of the locations where screwworms can be reared.

Basic studies of the type you propose are needed as background for the design of the next screwworm mass rearing facility. If the eradication program proceeds into Central America a new plant will be needed in Panama. This plant should be designed to operate with maximum efficiency for 100 years and should be capable of providing sufficient sterile flies to (1) maintain a barrier in the Darien, (2) eliminate any outbreaks that might occur north of the barrier, (3) eradicate infestations in the Caribbean and (4) undertake limited control or eradication projects in South America. Since labor costs can be expected to increase substantially in the future, a higher degree of mechanization would be justifiable in Panama. Best of luck with your project.

Sincerely,

Hugh

O. H. GRAHAM
Location/Project Leader

Enclosure - ARS Form 415
cc:
J. R. Johnston
R. A. Bram
H. E. Brown

COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO BARRENADOR
LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

SUBDIREC. DE PRODUCCION
DIRECCION MEXICANA
Km. 8 Carretera a Saltillo

SJMG/161/MMG

SEPTIEMBRE 24, 1984

DR. AGUSTIN PAGAZA GARZA
DR. RONALD E. LOWE
SUBDIRECTORES DE PRODUCCION

DR. ARTURO MARTINEZ Y T.
DR. DAVID HOFFMAN
JEFES DE INV. Y DES. EXP.

EN VISTA DEL ACELERADO AVANCE DEL PROGRAMA Y CONSIDERANDO QUE DESARROLLO DE MÉTODOS PUEDE REALIZAR ESTUDIOS QUE APOYEN EL COMPLETO ÉXITO DE LA COMISIÓN ES QUE A CONTINUACIÓN LES MENCIONAMOS VARIOS ESTUDIOS QUE PODRÍAN REALIZARSE POR PARTE DE LA PLANTA YA QUE CUENTA CON IMPLEMENTOS Y EL PERSONAL DE EXPERIENCIA NECESARIOS PARA ESTO, SIENDO LOS ESTUDIOS PROPUESTOS LOS SIGUIENTES:

A) DESARROLLAR ALTERNATIVAS QUE CON TIEMPO SUFICIENTE NOS INDIQUE CUANDO LA VARIEDAD EN PRODUCCIÓN SE ESTA DETERIORANDO.

B) INVESTIGACIÓN DE NUEVOS PRODUCTOS LARVICIDAS.

C) DETERMINAR MÉTODOS EFICACES EN UN 100% PARA LA SEGURIDAD BIOLÓGICA DE LA PLANTA PRODUCTIVA

D) DESARROLLAR DISEÑOS PARA TRAMPAS DE ADULTOS QUE PUDIERAN SER MÁS EFICACES QUE LA ACTUAL EN USO.

E) ESTUDIOS PARA MEJORAR LA EFICACIA DEL ATRAYENTE QUÍMICO DE ADULTOS U OBTENER OTROS NUEVOS.

F) OBTENER UN MÉTODO PARA DETERMINAR LA EDAD DE LAS LARVAS ENVIADAS A LOS LABORATORIOS DE DIAGNÓSTICO.

G) INVESTIGAR MÉTODO PARA DETERMINAR A LA ESPECIE ANIMAL QUE PARASITARON LAS LARVAS ENVIADAS AL LABORATORIO.

LEIBNITZ No. 20 - 12o. PISO TEL. 250-10-44 APDO. POSTAL M-2890 MEXICO, D. F. 06000

H) ESTUDIOS SOBRE LA CONDUCTA DEL PARÁSITO EN SU ECOLOGÍA NATURAL

I) INVESTIGACIÓN PARA ENCONTRAR O NO LA EXISTENCIA DE PREDADORES DE C. HOMINIVORAX EN CUALQUIERA DE SUS FASES DE DESARROLLO.

J) DESARROLLAR MÉTODOS PARA LA DETECCIÓN DE ANIMALES INFESTADOS A SU PASO POR LAS ESTACIONES CUARENTENARIAS.

AGRADECEREMOS CONSIDEREN LO ANTES EXPUESTO SOLICITÁNDOLES PRESENTEN UN PROTOCOLO DE CADA UNA DE LAS INVESTIGACIONES, DEBIDO A LA NECESIDAD DE INTRODUCIR LA METODOLOGÍA MÁS APROPIADA CON EL FIN DE MEJORAR EL CONTROL Y ERRADICACIÓN DEL PARÁSITO, DESEANDO TAMBIÉN SABER SI TIENEN ALGUNA OTRA IDEA QUE EN LA PRÁCTICA PUDIERA SER ÚTIL A LA COMISIÓN EN LA ETAPA QUE ACTUALMENTE PREVALECE.

ATENTAMENTE.

DR. SERGIO J. MARTINEZ GUZMAN
ENTOMOLOGO ASESOR

DR. HAROLD C. HOFMANN
ENTOMOLOGO ASESOR

C.C.P. DIRECTORES GENERALES
SUBDIRECTORES GENERALES
SUBDIRECTORES DE OPS. DE CAMPO

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plains Area
P.O. Box JJ
College Station, Texas
77841 – 5125

September 24, 1984

SUBJECT: Planning for Future Screwworm Research

TO: R. A. Bram
ARS Program Staff
Beltsville, MD

Ralph, again prior commitments will prevent me from joining you on November 8-10, 1984, for screwworm research planning at Tuxtla. I believe Paul Murrmann will be able to join you for this activity.

In addition to the agenda items, I am requesting that you take the lead in working with Hugh, Harold, and Paul and generate a firm plan for completion of the Veterinary/Entomology book that Hugh Graham and Manning Price have just about completed. We need to have a firm time schedule along with source and costs of publication.

Rex

J. R. JOHNSTON
Area Director

cc:
R. F Barnes/w/encl
R. P. Murrmann/w/encl
H. E. Brown
O. H. Graham
J. L. Giles

United States Department of Agriculture
Agricultural Research Service
Southern Region
Southern Plain Area
Screwworm Research
Apartado Postal #544
Tuxtla Gutierrez, Chiapas
Mexico

see mss. file for enclosures

September 25, 1984

SUBJECT: ARS Publication Policies and their application to a Manuscript by McInnis et al

TO: J. R. Johnston

You can quickly conclude from the correspondence on the subject (copies enclosed) that Dr. McInnis and I disagree about the present fitness of one of his manuscripts for publication. Although I forwarded the manuscript to a journal editor on February 17, 1984 so that the journal's editorial processes could begin, I did so on the condition that Dr. McInnis respond to all of the legitimate criticism provided by Dr. Elliott Krafsur in a peer review. Since that time there has been some confusion in the review and approval process, primarily because I only recently saw a memorandum which Dr. McInnis had prepared on March 20 at my request. Prior to seeing his memo I had recalled the manuscript from the Editor because (1) Dr. McInnis had submitted it as being in final form without my concurrence and without changes proposed by a co-author (Mackley), (2) he had not followed a major recommendation from two peer reviewer and from me that the paper be substantially shortened, (3) he had requested that the galley proof be sent to him in Honolulu, presumably for him to handle without reference to his co-authors (I had asked that Dr. Mackley correct the galley proof because McInnis had previously embarrassed our group with his careless handling of another screwworm galley proof; the paper McInnis et al 1983, Ann. Entomol. Soc. Am. 76:628-40 contains an inexcusable number of typographical errors which should have been caught on the galley) and, (4) the paper as returned to the journal by McInnis in July contained misspelled words and some misstatements of fact (minor misstatements for the most part, but they should have been corrected).

Now that I have seen the McInnis memo of March 20 I feel that it is little more than a rebuttal and that he did not accept certain valid criticisms by Dr. Krafsur that he should have used to improve the paper. In addition I cannot detect a serious effort by McInnis to follow the suggestions of two journal reviewers. The version which I recalled is verbose and reflects McInnis's lack of information about progress of the program since he left it in 1982. More importantly McInnis has rejected attempts by Mackley to reorient the paper so that it would be an objective presentation of the data obtained and emphasize the one rather simple conclusion that can be drawn from those data.

My comments about the deficiencies in the July 13 version that I recalled have been written on the enclosed copy of that manuscript. As Krafsur said, it promises more than it delivers. McInnis's approach was too broad; he started out by reviewing several earlier field evaluations without regard to their pertinience to a study of the effect of immigrating wild flies into a field test area. The title should be more specific and the introduction should be rewritten to focus attention on the main thing the paper has to offer — some valuable information about the

proper size of a field test area and the need to keep the sentinel sheep pens away from the outside edges. With the verbosity eliminated the paper would be substantially shorter and much easier to read and understand.

If I am correctly reading Dr. McInnis's letter of August 15 to me, he is saying that he as senior author is rejecting my recommendations for shortening and correcting the paper and that he will not make any further changes in it. His statement about privately financing publication of course ignores the fact that the data are not his personal property; they belong to ARS and the best interests of ARS must prevail over the personal pride of any of its employees. Also, I disagree with his statement that only the senior author can reconcile different viewpoints and write the final version of the paper as it will be published.

I have made several mistakes in my handling of this paper. In the first place, I should not have approved senior authorship by McInnis; the other authors did more of the work than he. I should not have sent the manuscript to you or the journal editor with a request for approval for publication until the obviously needed changes had been made. I erred in thinking that McInnis would work with me and his co-authors and improve the paper as it went along. We all feel a certain pressure to handle manuscripts rapidly and not delay their publication, but I will in the future be more insistent that my criticism and suggestions receive positive consideration and will be much slower to submit manuscripts to you with a recommendation of approval. Contrary to the philosophy expressed by McInnis, I am personally proud of high quality screwworm research publications and chagrined by the publication of poor quality papers. Whether I am an author or not, I have supervisors in ARS and peers in entomology who will say, "Hugh Graham was sleeping on the job when he let that one get by" if the paper is obviously poorly written.

Your approval dated 2-11-84 was conditioned on the "assumption that Professor Krafsur's concerns have been properly addressed". Since in my opinion they have not been, I recommend that you ask Dr. McInnis to rewrite the entire paper and to respond to the criticism of three peer reviewers, one co-author and one research leader, not just rebut that criticism. If he is not willing to do this, he should either accept a junior authorship or withdraw completely and let Mackley and Peterson publish the paper.

OHGraham

O. H. GRAHAM
Location/Project Leader

cc:
H. E. Brown
R. A. Bram
J. W. Mackley

Enclosures:
1 - Area O transmittal
2 - Informal note
3 - McInnis memo of 3-20-84
4 - letter, OHG to Everngam
5 - letter of 8-15-84, McInnis to OHG
6 - letter of 8-29-84, McInnis to Mackley
7 - peer review by Krafsur
8 - journal peer review # 1
9 - journal peer review # 2
10 - mss. with corrections

United States Department of Agriculture
Agricultural Research Service
Northeastern Region
Beltsville Agricultural Research Center
Beltsville, Maryland 20705

September 25, 1984
(Ref.: Lot 84-07740)

SUBJECT: Insect Identification

TO: William Rubink
USDA, ARS, SR
Screwworm Research
P.O. Box 267
Weslaco, TX 78596

The enclosed identifications have been provided by Research Entomologists of the Systematic Entomology Laboratory, USDA, and represent a partial report on the specimens submitted with your request received August 9, 1984.

As additional identifications become available, we will report again.

The specimens, except as noted, will be returned under separate cover. Thank you for the specimens kept for the U.S. National Collection.

The specimens have been numbered for convenience of reference. Please note that in future submittals specimens must be individually numbered to facilitate reporting of identifications.

for LLOYD KNUTSON, Chairman
Insect Identification and Beneficial
Insect Introduction Institute

Enclosures

Separate cover: Specimens

IDENTIFICATIONS LOT 84-07740

DIPTERA

Muscidae

2,3 - Atherigona orientalis Schiner	2
4,5 - Ophyra aenescens (Wiedemann)	2
26 - Synthesiomyia nudiseta (Wulp)	1
7 - Morellia basalis (Walker)	1
6 - Neomuscina sp.	1
Group needs revision; species are not identifiable.	
NO# - Too poor for determination	1

Determined August 13, 1984 by R. J. Gagné

Sarcophagidae

*11 - Sarcophaginae - Genus undetermined	1
*12 - Sarcophaginae - Genus undetermined	1

Calliphoridae

9,10,27 - Hemilucilia segmentaria (Fabricius)	3
16,17 - Chrysomya rufifacies (Macquart)	2
18-21,24,25 - Chrysomya megacephala (Fabricius)	6, 3 kept
1,13-15,23 - Cochliomyia macellaria (Fabricius)	5
22 - Phaenicia eximia (Wiedemann)	1

*Group needs revision; species are not identifiable.
I suggest you write to Dr. Hugo de Sousa Lopes, Academia Brasileira de Ciencias Rua Anfilofiode Carvalho, 29 3° Andar Caixa Postal 229, 20.00 - Rio de Janeiro, RJ, BRAZIL for permission to forward your specimens for possible further determination.

Determined August 13, 1984 by N. E. Woodley